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THE JOURNAL OF PHILOSOPHY

THE PRINCIPLE OF PARSIMONY IN EMPIRICAL SCIENCE

SELDOM in the history of thought has one man's name become so universally attached to a principle which he did not formulate as has that of William of Occam. Hamilton called the demand for simplicity in explanations "Occam's Razor," and this name has remained as a testimonial to Hamilton's felicitous phrase-making rather than to his otherwise great acumen as an historian of thought. His designation has survived in spite of the fact that the principle to which he attached it was not formulated until about three centuries after that philosopher died.[1]

Nevertheless, though Occam probably did not say *Entia non sunt multiplicanda praeter necessitatem*, he *might* have said it, and there is some justification for giving him some credit for it. Though many a philosopher before him had objected to an opponent's ideas on the ground that they were not sufficiently simple (for example, Aristotle's criticism of the classical theory of ideas), few philosophers have been in greater need of such a principle than some of Occam's opponents, and few philosophers as ruthless as he in cutting away what he thought were metaphysical extravagances.

The attribution of this principle to Occam, even though not vindicated by historical fact, is symbolical also of two characteristics which have attended its later use. First, it has almost always been explicitly associated with nominalism, due to the *prima facie* simplicity of the latter in metaphysics; and, by contagion, one may say, it has characterized most antimetaphysical systems because of their relation to sensationism which historically grew from nominalism. Secondly, the use of the principle has been largely polemical. It is most apparent in polemics and dialectic, not in orderly systematic thinking from established premises. Naturally it appears often in controversies over relative degrees of simplicity, and many a thinker has accused his opponent of extravagance when perhaps his own work might not have served as

[1] For a more accurate account than that of Hamilton see W. M. Thorburn, "The Myth of Occam's Razor," *Mind*, Vol. XXVII (1918), pp. 345–353.

a perfect exemplification of the rule. One reason for these controversies lies in the fact that the principle is so abstract that it warrants divergent applications,[2] and other reasons will become apparent as we proceed.

These two controversial aspects of the application of the rule constitute the subject of this essay. We shall attempt to answer two questions:

(1) Is the principle of parsimony applicable in a definitive and unique way, permitting a decision as to the value of conflicting scientific theories?

(2) Does the principle of parsimony have any realistic, i.e., objective or cosmological, implications for the material to which it is applied?

These questions can not be handled separately. The answers to them will come out in an examination of the diverse formulations and justifications of the principle itself. There are various ways in which the principle may be made plausible, and the implications of each must be examined. These modes of conception we shall call the *metaphysical,* the *naturalistic,* the *formalistic,* and the *conventionalistic.*

I. The Metaphysics of Parsimony

The demand for simplicity in scientific formulations is, at least in modern times, an historical outgrowth of a particular metaphysical theory, in which the Christian doctrine of the unity of the world and the Greek belief in its intelligibility formed the stage on which new mathematical and empirical interests produced a renaissance of science. Originally the demand for simplicity was not antimetaphysical; it was a principle within speculative philosophy, and Occam was particularly attacking a theory of reality which he considered to be extravagant metaphysics.

The methodological aspects of simplicity were only corollaries of this, for if nature were simple, concepts of it should likewise be simple. It is in line with this tradition that Newton both renounces metaphysical hypotheses and yet appeals to a realistic cosmological, and even a metaphysical, basis for his attempts to simplify the scientific picture of reality. In a famous passage he says,

> We are to admit no more causes of things than such as are both true and sufficient to explain their appearances. To this purpose the Philosophers say

[2] Cf. J. W. Nogge, "Regarding the Law of Parsimony," *Journal of General Psychology,* Vol. XLI (1932), pp. 492–494.

that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

Specifically, this search for simplicity involves a reconstruction of both science and metaphysics; instead of a "vertical relation" between hidden causes (occult qualities in the Aristotelian system) and visible effects, science should seek to establish "horizontal relations" between various observations; this is the meaning present in his famous *Hypotheses non fingo*. Obviously, however, Newton could not be a positivist, so his principle of exclusion of causes is not a limitation of scientific concepts to direct observation, but lies in the answer to the question, "Does the assumption of such and such a cause permit mathematical prediction of observation?" This question changes the direction of metaphysical interest from speculation beyond experience to a justification of experience by its own resources critically examined and extended where necessary. Cooley has shown that in modern times, when metaphysics has thus become largely a handmaiden to science, metaphysical simplicity has not been so much a logical basis of the methodological rule as an attempt "to make simple explanations possible [in a realistic sense] by conceiving of the subject matter in such a way that simple explanations will fit it."[3]

Consequently, it is difficult to speak of a metaphysical derivation of the rule of parsimony. Rationalism, the leading example of the development Cooley mentions, is not so much a metaphysical justification of the new science as it is its metaphysical consequence, though both the metaphysics and the science strengthen each other by their inner harmony in dealing with one world. In the long run, as it ends in positivism, this relationship leads not only to a simplification and impoverishment of metaphysics, but eventually to its renunciation, when to speak of justification is an offense to the positivistic thinker. And after Hume and Kant had shown the independence of method from any particular presuppositions in cosmology, the need for a metaphysical justification of methodology *from the side of the object* disappeared. But the contribution of Hume and Kant opened the way for a new kind of metaphysics and a new route of justification. From them comes the modern preoccupation with the rôle of the scientist rather than with that of his object; from the former's psychology come attempts to justify method on psychological grounds, and

[3] W. F. Cooley, "The Lure of Metaphysical Simplicity," *Studies in the History of Ideas*, Columbia University Press, New York (1925), Vol. II, pp. 161–182.

from the latter's logic there arises at least the incentive for, if not the guide to, a formalistic defense of methodology.

II. Naturalistic Theories of Parsimony

The term "naturalistic" is used here in a broad sense to cover all theories of methodology which regard economy as a consequence of the natural functioning of the mind in scientific thinking. Such interpretations were suggested by Hobbes, Hume, and Mill; they become almost inevitable after Darwin, and they reach their classical formulation in the work of Mach.

Mach was led to his theory by three chief considerations. The first was that the history of science (as traced in his great history of mechanics) was a continuous development in the direction of more and more simple representation of observations; great discoveries in science are not so much new observations, but new simplifications in the interpretation of old facts. His account of the development of the concept of mass is a perfect illustration of this tendency in science. Second, "minimal principles," for example, those in optics and mechanics, impressed him as being one of the highest kinds of scientific generalizations, and he regarded science itself as a "minimal problem." Finally, his debt to the thinkers mentioned previously was his conviction of the dominant rôle of control in science and of the importance of the fact that science is to be evaluated as a tool.

These three factors led him to his view that the construction of a scientific theory was a process of finding abstractions to cover the widest variety of observations "with the least mental effort." "Science itself . . . may be regarded as a minimal problem, consisting of the completest possible presentment of facts with the least possible expenditure of thought."[4] When we comprehend a thing without effort, " it is *explained* for us," and this comprehension is equivalent to "discovering everywhere the same facts."[5]

Two objections to Mach's theory have rendered it untenable at least in the relatively simple form in which he presented it; and though it perhaps stands as a description of the parsimonious tendency and attitude of the scientist, it is in no sense a justification of them.

The first is that the kind of simplicity Mach was seeking was inconsistent with another kind which might be equally desirable,

[4] E. Mach, *The Science of Mechanics*, English translation, Open Court Publishing Company, Chicago (4th ed., 1919), p. 490.

[5] E. Mach, "The Economical Nature of Physics," English translation, *Popular Scientific Lectures*, Open Court Publishing Company, Chicago (1919), p. 194.

and the incompatibility between them makes any absolute reckoning of simplicity on his basis impossible. Mach was seeking to simplify scientific description, to "find the same facts everywhere," but in order to achieve this, he had to go against an economy which would not permit multiplication of entities. (The opinion that these entities have only a fictional status and that the only real things are sensa is important as a metaphysical proposition; but the scientist has to use atoms and the like, and to call them fictions does not simplify his thinking.) Such entities or fictions as atoms immensely complicate a simple system based on sensationism. Thus Mach had to complicate one aspect of his work in order to show how the mind attains to its natural destination in simplifying its *prima facie* objects. If Mach's system is justified, then, it must be on pragmatic grounds of control through simplicity of formulation rather than on the basis of representing a maximum simplicity in general.

The second objection to the naturalistic theory has been stated in its classical form by Husserl. This well-known argument need not be repeated. Suffice it to say that he exposes the psychologistic basis of the theory, and then refutes all psychologism in science by showing how it eventuates in scepticism. Economy of thought, he admits, may be a fact of nature; this, however, does not justify the logical consequences seemingly derived from it, and its regulative and normative use is inconsistent with its own descriptive basis.

> A psychological or epistemological law which speaks of a *striving* to accomplish *as much as possible* is an absurdity. In the pure sphere of facts there is no "as much as possible," in the sphere of law no striving. And from a psychological standpoint there is in every case something definite, just this much and no more.
>
> To identify the tendency to the highest possible degree of rationality with a biological tendency to adaptation or to derive the former from the latter, and yet to impute to it the function of a basic psychological force—that is a mass of confusions which finds its parallel only in the psychologistic misinterpretations of logical laws and in their conception as natural laws.[6]

Nevertheless, Mach was insisting upon an important fact, that science does reduce things to their lowest common denominators and substitute the latter for the former, obtaining greater generality than would be possible with unreduced observations, and saving thought by substituting a few rules for many unique observations. The extent to which this is important as guiding methodology will be determined later.

[6] E. Husserl, *Logische Untersuchungen*, Niemeyer, Halle (1913), Vol. I, pp. 204, 207.

III. Formalistic Derivations of the Principle of Parsimony

The formal justification of the principle of economy did not arise historically as a result of Husserl's attack on Mach, but it can nevertheless be best understood as a substitute for the discredited liaison of naturalism with positivism (as this was assailed almost simultaneously by Husserl, James Ward, and others). By the formalistic theory I mean the view that the principle can be regarded as a logical formula, derivable like other principles such as that of identity from a few basic axioms which are not themselves normative but which have conclusions which may be applied regulatively. Such derivations must be regarded as valid; their usefulness in directing or judging scientific work, especially in empirical fields, is, however, more doubtful.

Whitehead has remarked, "As hypothesis increases, necessity diminishes."[7] Erich Becher, attacking the fictionalists, was perhaps the first to derive the principle from considerations of probability, for he held that it was applicable without qualification only to fictions, where probability (usefulness in prediction and nothing else) was at stake.[8] Hailperin[9] showed that

$$P(p \cdot q) \leqq P(p)$$

which he called "essentially the basis for Occam's Razor," though actually it is, of course, only a necessary condition, and he does not prove the sufficient condition.

Nelson[10] went further and showed that, if we assume (*a*) explanation consists of one or more independent hypotheses, (*b*) hypotheses consist of groups of propositions in mutual implicative relations (i.e., hypotheses are independent of each other), and (*c*) the independent hypotheses have equal *a priori* probability, then

$$P(E) = P(H_1 \cdot H_2 \cdot \cdots \cdot H_n)$$

and

$$P(H_1) > P(H_2 \cdot H_3).$$

[7] A. N. Whitehead, *The Organisation of Thought, Educational and Scientific*, Williams and Norgate, London (1917), pp. 175–176.

[8] E. Becher, *Die Voraussetzungen der Naturwissenschaften*, Barth, Leipzig (1907), pp. 19–20.

[9] T. Hailperin, "Foundations of Probability in the Mathematical Logic," *Philosophy of Science*, Supplement, Vol. IV (1937), pp. 125–150.

[10] E. J. Nelson, "A Note on Parsimony," *ibid.*, Vol. III (1936), pp. 62–66. (I have simplified the symbolism in this brief account.) It should also be noted that the deduction outlined above is by no means the whole of Nelson's contribution to the problem. He states that this is not the only or the most important type of simplicity, and among the others he lists: number of entities assumed; type, degree, and number of relations or functions; simplicity of numerical coefficients, and a number of terms in polynomials.

This proof depends only on the multiplicative axiom in probability theory.[11]

We have said that this connection between probability and simplicity has been formally established. As thus proved, it is of immediate application in mathematics, though probability itself can not be taken in any sense other than "likelihood of involving inconsistency (error)." In mathematics, simplicity is absolutely required. Since independent assumptions are made and their consequences must be self-consistent, and since we can not be sure of meeting these requirements in any absolute sense (but only through eventual demonstration of an isomorphism between a subset of the postulate system with a part of an *assumed* paradigm of simplicity such as the real number system or the Euclidean geometry), the danger of inconsistency is reduced by assuming as few independent starting points as possible, for it is within them that latent conflicts may be hidden.

But in empirical science, the conditions which would render Nelson's deductions applicable do not ordinarily, and perhaps never, obtain. We can see this in three ways: (*a*) The condition of complete independence of hypotheses is not fulfilled and is not desirable. (*b*) The condition of equal probability does not obtain in an empirical science. (*c*) The field of application to which hypotheses are applied is not a limited one as in a deductive science wherein the field of application (the scope of the theory) is generated by implications of the postulates; where this basic difference between a deductive and an empirical science is overlooked for the sake of simplicity of construction, science becomes dogmatic.

(*a*) The hypotheses making up a scientific theory are not completely independent so that their probabilities could be separately determined. Take, for example, the basic assumptions of Newton's system. It would be erroneous to suppose that one could be deduced from the other (within Newton's system). Yet it would be equally erroneous to call them independent in the sense that their plausibilities (or probabilities) are irrelevant to the con-

[11] Another formalistic deduction of the rule is independent of considerations of probability, and is capable of being extended to a realistic objective basis, but it is not derivable simply from the principle of sufficient reason. It depends upon two premises: (i) Every event has a sufficient reason; (ii) Every event is the sufficient reason for another event. Then A has a sufficient reason a, and a is alone sufficient to explain or cause A. If, however, $a + b$ is assumed, then the effect by (ii) will be $A + B$; but if the effect is A, then to assume $a + b$ as the cause is inconsistent with (ii). Therefore, every increase in the complexity of a cause increases the complexity of the effect.

joint assumption of the entire set. The definition of time, for example, is not derivable from that of space, yet it would be materially improbable that one would be of use without the other; the second law of motion, furthermore, is improbable without the first, yet it is not deduced from it. To break them down into their truly independent parts and then to judge the system on the basis of its simplicity would certainly be foreign to the grounds on which Newton's system recommended itself for two centuries as the simplest cosmology. It was certainly not this sort of simplicity which was sought by Newton; his desire for simplicity was largely directed against the loose ends of Kepler's work and Descartes' extravagance in physical models.

Another example of this same interdependence in the "axioms" of empirical science is obvious from an analysis of Darwin's system. From the existence of life and variations and the concepts of overpopulation and heredity, Darwin derives the principle of struggle for survival, natural selection, survival of the fittest, and the consequent establishment of new kinds. This is not to say that the theory is not less simple than the one it replaced, but merely to point out that its simplicity is not discovered by *counting* postulates or independent hypotheses; it is simplest because its starting points are not only few in number but also because they are of "one kind," with none of the discontinuity and arbitrariness of the various subhypotheses of the theory of special creation.[12]

(*b*) What has been said about the interdependence of the hypotheses or concepts implies the second objection, namely, that the requirement of equal probability can not be met with in practice. Yet it is necessary for the establishment of the formal minimal principle, because three hypotheses each with a probability of .99 have a total probability higher than two with probabilities of .50, even though the total explanation is numerically more complex.

(*c*) The scope of a scientific theory is not limited by what is implicit in its postulates, but is also determined by the kind of facts to which the theory is later exposed and to which it ought to apply. (Since the postulates serve as criteria of relevance, however, it often happens that they do limit the scope of the theory in a way not empirically warranted.) For these reasons, scientific simplicity is always tentative and is not something that can be judged once and for all. What is rightly judged to be a simple

[12] Popper's concept (*Logik der Forschung*, Springer, Vienna, 1935, p. 94) that simplicity is equivalent to "falsifiability" and thus a positive function of empirical content is a better reason for preferring Darwinism than any counting of hypotheses.

theory one day may have to be acknowledged complex the next by the enforced addition of an *ad hoc* hypothesis occasioned by an "interference" from the outside where none was expected. One may say formally, of course, that theory *A* plus *ad hoc B* is not the same theory any longer, and in a purely deductive system where all hypotheses are logically independent it would not be. But in an empirical science where this independence does not obtain, the *ad hoc* in effect belongs to the system, since its very suitability to the tone of the whole has made it acceptable in place of the infinite variety of *ad hoc* hypotheses theoretically available if the requirement of complete independence is fulfilled.

A scientific theory is to be judged not only on grounds of its internal simplicity, but also from the standpoint of its relevance to other similarly simple systems. Here, as indicated previously with regard to Newton's laws, we do not have either complete independence or complete inferability. Naturally, the entire set should be as simple as possible, and the synoptic function of great theories in science is to tie lesser theories together. But often we are not using the entire set; and in such a case we do often prefer a more complex hypothesis than is required by the particular problem in order to achieve relevancy to a larger field of scientific experience. Peirce,[13] for example, limited considerations of simplicity not to the probability of the separate components of a theory but to its "total probability." Thus, he says, it may be more parsimonious to explain any specific human action in terms of pure egoism, but it is more probable that, remembering the wide variety in behavior, there are other causes, and an admixture of other motives into the explanation of a single action should not be ruled out by too narrow an application of the rule of simplicity. Wertheimer [14] goes so far as to distinguish between the truth-values of partial and systematic propositions.

Still another danger in the use of the minimal principle is apparent from the work of Hugo Dingler. A short introduction to one aspect of his thought will serve to clarify the following criticisms. Dingler is an "operationist" and is often called a "conventionalist," though he renounces this appellation with good reason. He holds that the apriority of scientific propositions lies in the fact that they are all defined by a finite number of repeatable operations. For them to be uniquely defined, some formal principle must be agreed to which will uniquely determine the opera-

[13] C. S. Peirce in Baldwin's *Dictionary of Philosophy and Psychology*, article on "Parsimony."

[14] M. Wertheimer, "On Truth," *Social Research*, Vol. I (1934), pp. 135–146.

tional series by which they are defined. The formal principle is that of simplicity, which is thus derived from the requirement of uniqueness. "The solution which is suitable," he says, "is the only one which can be uniquely defined by the methodological means then available. Such solutions have then naturally the characteristic of also being the logically simplest, that is, they require the fewest determinations."[15] Now, Dingler continues, the simplicity which such a solution has is purely systematic or internal (*innenbestimmte Einfachstheit*), because it is a simplicity resulting from a concatenation of the only possible operations with the fewest possible *Grundfähigkeiten.* It does not coincide, however, with what is frequently desired, a simplicity in application to a particular case, or *aussenbestimmte Einfachstheit.*[16] For example, the unique determination of a plane is a necessary condition for the reproducibility of a measuring instrument, and it is uniquely determined only in Euclidean geometry, which, in turn, is based on a minimum of *Grundfähigkeiten,* namely, the acknowledgement only of distance and the plane. Therefore, he holds the "methodological means" at our disposal at any time to be Euclidean, and measurements, to be systematically simple, must then be interpreted in a Euclidean manner. This may, however, complicate actual calculations and predictions (or render them impossible). But such criticism, made from the standpoint of external simplicity, he regards as invalid, or at most as only temporarily justified by the exigencies of "front-line science," since, if taken systematically (as he objects that Einstein does), it destroys the foundation of the instruments by which both systematic and external simplicity are reckoned. He is especially critical of those who regard "measurement as an external absolute."

The effect of this sharp distinction between internal and external simplicity is to make a breach in the unity of a scientific theory, since Dingler admits that "front-line science" can not be restricted always to the systematically simplest procedure. The new discoveries must often be taken care of *ad hoc,* meaning that the total complexity of the theory is increased. When this is decreased by a systematic integration of the *ad hoc,* there is a consequent loss in external simplicity. There is no other alternative.

An increase in the number of presuppositions or basic operational possibilities is needed to avoid the conflict between generality and simplicity which is engendered by Dingler's *a priori* decision as to what is to be permitted. If his original presuppositions,

[15] H. Dingler, *Die Methode der Physik,* Munich, Reinhardt (1938), p. 86.
[16] *Ibid.,* pp. 284–285.

which need not, of course, be self-evident or psychologically prior, were those of a more general geometry, subsets of the hypotheses would be applicable within different ranges and the molar instruments would not have to be calibrated for microscopic and macroscopic deviations from Euclid. This would necessarily lead away from his numerically minimal simplicity, but the effective simplicity (a function of the presuppositions, the total number of determinations necessary, and the shortness of the deductive route from presupposition to observation) could be reduced to far less than it is when Dingler attempts in terms of his neo-classical physics to treat the phenomena on which relativity is based.

There is another systematic danger in Dingler's use of simplicity which would be avoided were he less of an absolutist and more the conventionalist which he is often considered to be. This is the threat of dogmatism (which he does not escape) resulting from an *a priori* determination of the structure which a scientific theory must have. That is, he allows his rule of exclusion of operations to become the general rule of the incorrigibility of postulates, which is characteristic only of a deductive science in which everything may be presumed to be known *ab initio*. This transition, which is made in the name of uniqueness or simplicity, actually reduces the data to an entirely secondary position, making physics a formal science. Fruitfulness is replaced by elegance; the physicist need not go to the laboratory. To appeal to simplicity becomes itself merely an assumed maxim, and the absoluteness which he seeks proves itself to be merely arbitrary convention.

IV. A Conventionalistic View of Parsimony

Simplicity may be regarded merely as a desideratum, an ideal which should be sought but whose locus is not fixed *a priori*. Naturally, each of the other accounts of the principle which we have surveyed has its normative aspects too, yet in each case the principle has been derived from something which does not purport to be normative—from some theory of the nature of the object or the nature of man or from a mistaken identification of the conditions of formal with those of empirical thinking. In each case, due to the formalism and abstractness of the derivation, either the demand for simplicity was carried so far as to nullify some of the other desirable features of a scientific system, or the conditions required for the application of the principle could not actually obtain. If we do not derive the law of parsimony from extra-scientific considerations, but regard it exclusively as a rule of the game, refusing to acknowledge the legitimacy of an explanation which assumes

more than is necessary for resolving the particular *type* of problem before us, we have what may be considered the conventionalistic theory of parsimony.

It is obvious that simplicity is not the only convention of this character. Formally distinct from it, but equally difficult to justify in any of the ways mentioned previously, is the principle of sufficient reason.[17] A third requirement would be that of generality; due to the systematic nature of science, an explanation of a single observation must be adequate to it and also contain any complications necessary to render it applicable to a reasonable maximum variety of other observations; an "explanation" of one fact alone is a misnomer. It is clear that the principle of simplicity is now very restricted and can be applied effectively only after the adequacy of a set of hypotheses has been established.[18] An overly conscientious use of the principle of parsimony may be expected to lead to theories which are too narrow, since every scientific convention becomes in time a principle of exclusion or a criterion of relevance and appears to the careless thinker as a natural law.

Simplicity in a closed field of deductive science may be regarded as a final attribute; in empirical science, of which we may say not only a minimum of assumptions but also a maximum of observations are needed, it is always relative to generality and adequacy. Adequacy we may regard, in its turn, as like an Hegelian synthesis of the principles of sufficient reason and parsimony; it is a demand we make on science because the two principles are, in isolation, incompatible in their effects on scientific thinking. There is a conventional dialectic resulting from this antithetical relationship and the constant leaven of new observations which should not be inhibited by either.

The historical transition to the heliocentric astronomy may serve as an example of this.

Copernicus is quite explicit in his attempt to introduce a greater

[17] The conventionalistic relation between parsimony and sufficient reason has been stated by Kaufmann with what I consider to be parsimony basic. He says, "The rules of scientific procedure contain the *conditions for an exemption from the general prohibition to change the corpus of a science.*"—Felix Kaufmann, "The Logical Rules of Scientific Procedure," *Philosophy and Phenomenological Research*, Vol. II (1942), pp. 457–471, esp. p. 459. It would seem to me that the name for the "general prohibition" would be not "the methodological principle of sufficient reason" as he suggests, but the principle of parsimony (or exclusion); the rules determining exemptions express sufficient reasons. In another essay I hope to examine critically various theories of the status of the law of sufficient reason.

[18] Lloyd Morgan says, "We do not know enough about the causes of variation to be rigidly bound by the law of parsimony." *Animal Life and Intelligence* (1891), p. 174.

simplicity into the Ptolemaic theory.[19] Kepler found, however, that the systematically justified parts of the Copernican system (i.e., those parts not treated analogously to the Ptolemaic system, which in fact are the forty of the original eighty odd cycles which Copernicus could replace) were *too simple,* and that this was the cause of the necessity of assuming the remaining *ad hoc* epicycles. Through a fundamental change in the postulate of circular orbits, by which this concept was replaced with a more complex elliptical concept,[20] he increased the field of the systematic concepts without the use of *ad hoc* hypotheses. The systematic part of his work, however, is less parsimonius than the systematic parts of Copernicus' work.

It is thus quite apparent that the formal principle with its minimal numerical concept can not be very relevant to empirical work. Even within some parts of mathematics, where certain results are intentionally sought, simplicity of the axiom system must also be tempered by the desire for adequacy. In this case, the locus of simplicity is shifted from the axiom set (which is logically independent of the particular consequence we wish it to justify) to the total solution of the problem, since, if the axiom set is inadequate, hypotheses *ad hoc* would be necessary for the particular solution. If these latter are to be systematically justified, the total complexity is increased or the entire system regarded as a subset of some more general axioms.

But the problem of how high a price (in *ad hoc* hypotheses) we are willing to pay for axiomatic simplicity is a central one in empirical science. To show this, let us take two cases, one decided and one still under debate.

In physics, the Newtonian system predicted a positive result for the Michelson-Morley experiment. None was found. Three ways were open to the physicist. (i) He could give a nonphysicalistic hypothesis *ad hoc* which would save the simplicity of the system. He could say there was an experimental error and forget

[19] I refer here not to the passage in the preface in which it is stated that the hypothesis is put forward only to facilitate calculations, since this is now regarded as spurious.—Cf. H. S. Jones, "Copernicus and the Heliocentric Theory," *Nature,* Vol. CLI (1943), p. 573. The reference is rather to the argument that it is simpler to suppose one globe to move and many to be stationary rather than the reverse.

[20] Popper (*loc. cit.*) mentions a similar case as one of his examples to show what he means by simplicity. Since it takes fewer determinations to decide whether a given set of points lie in a plane than for determining whether they are on a quadratic surface, the "linear connection is simpler than the quadratic." That is, a linear connection is more easily "falsified" than a higher curve surface.

the experiment. To follow this path means, however, ultimately to regard the laws of physics as "legislative" for nature and to render science dogmatic. (ii) He could formulate a physicalistic hypothesis *ad hoc,* which would leave his basic system simple but adapt it to this particular contingency. Thus he could suggest the Fitzgerald-Lorenz contraction hypothesis. This, however, when looked at critically is, of course, an increase in systematic complexity, since to the extent that it is logically independent it ought to be put at the beginning with the laws of motion instead of *ad hoc* at the end. (iii) He could try to show that the *ad hoc* hypothesis and the axioms are not logically independent but rather special consequences of another hitherto unformulated axiom set which may have either fewer logical independents or more basic suppositions whose interconnections give them a plausibility which the Newtonian axioms together with the *ad hoc* contraction hypotheses do not have. This was the course of Einstein, who says that the inadequacies of the Newtonian system "are not logical objections to the theory. They form, as it were, merely unsatisfied needs of the scientific spirit in its effort to penetrate the processes of nature by a complete and unified set of ideas." [21]

The other example is taken from so-called "psychic research." The existence of para-psychological phenomena is still under debate, and what is said here is not intended as suggesting a positive or negative decision on this question; it, like the one in physics just described, is a technical matter for experts alone. But the behavior of psychologists in this field is relevant to our considerations. There have been systematic objections to Rhine's work, but in addition to these (which do not concern us here) there have been purely defensive reactions from the side of many psychologists who reject the results because of their apparent inconsistency with deductions from accepted principles or presuppositions. One psychologist has said that he will never be convinced of the existence of a psychical phenomenon by any experiment whatsoever, because he knows *a priori* that it is impossible. To support this exclusion, which he defends on the basis of simplicity, since to take Rhine's results seriously would require either a new postulate or a restructuring of the whole, he may be forced more and more to bridge the gap between theory and the experimental observations by *ad hoc* hypotheses. These may include some legitimate psychological concepts; in this case they add to the total complexity of

[21] A. Einstein, "Isaac Newton," *Annual Report of the Smithsonian Institution,* 1927, pp. 201–207. Cf. also J. H. Jeans, "Newton and the Science of Today," *Proceedings of the Royal Society,* Series A, Vol. CLXXXI (1943), pp. 251–262, especially pp. 252, 258–259.

his theory. Or they may be nonscientific, as the accusation that marked cards were used, etc. What makes such a standpoint as this relevant to our purpose is that it will either replace science with dogmatism or else eventually force the question which we have seen the formalist to be unable to answer: How complicated can a theory become at the conclusion level without forcing a restructuring at the premise level? Obviously, no mathematical answer such as "One premise is worth five hypotheses *ad hoc*" can be expected; but unless a fairly definite answer can be given in a particular case, simplicity in the sense of the formalistic minimal or numerical concept is no guide to scientific construction.

Simplicity, it seems to me, is more a matter of "scientific strategy" or "tact" in judging the value of a shift in the base of operations. If it is so, we can not derive hard and fast rules for its application, nor can we regard it as determined finally for each theory, thus permitting a choice among them on this ground alone.

There are several inferences which are justified by this concept of simplicity.

(1) We have not discussed the recent psychological accounts of the principle of simplicity in which the pattern-character of perception is taken as the natural ground for simplicity in science. I did not mention them because, in principle, they resemble Mach's work too closely. But in conjunction with the conventionalistic theory, they become very relevant, since they are an acknowledgment that suitable conventions do have a psychological basis. What appears suitable at a given time will depend in part upon the "intellectual climate," the scientific environment, and the character of the scientific thinker. This gives a basis, even if a variable one, for estimating the relative simplicity of two theories. R. B. Lindsay, having also concluded that formalistic criteria of simplicity are inadequate, states that the simpler of two theories is the one with which a normally intelligent person can, in the shorter time, become sufficiently familiar to obtain correct and useful results.[22] While this may not be the kind of interpretation we expect from one of the most honored and ancient traditions of methodology, as a "rule of thumb" it is probably as good a way as any for determining the "effective simplicity" in an empirical science. It must be borne in mind, of course, that this kind of simplicity may not coincide with logical or formal simplicity, and that for the latter type there is no substitute in a deductive science, no matter how much the difficulties in understanding may be multiplied by the maxim.

[22] R. B. Lindsay, "Simplicity in Physics," *Philosophy of Science* (1937), Vol. IV, pp. 151–167. See especially p. 166.

(2) The type of simplicity within a scientific system will depend upon the purpose of the explanation. If sciences are distinguished not so much by the answers they give as by the questions they ask, it is obviously unimportant to arrange them in order of simplicity, as attempted by Comte and Spencer. It is also obvious that some sciences use the concepts of others, that this borrowing is not always mutual, and that where it is not, the science which borrows is to this extent dependent on the other and formally less simple. For example, the conceptual field of biology is less simple than that of physics to the degree to which physical laws are presupposed within the scope of the biological questions. We naturally prefer, when possible, a physical explanation for most purposes, but often we are content with sciences which are "autonomous" only to the extent that they are simple in Lindsay's sense. Still, to prefer physics as a matter of principle because of its minimal simplicity is erroneous. It is even more groundless to equate its conceptual simplicity with its truthfulness outside of the context of a specific type of problem and to admit into cosmology only what is physicalistically defined.

(3) Simplicity has appeared to some writers to be primarily an esthetic category. As long as scientific simplicity was read in terms of the formalistic minimal principle, such suggestions could only be tolerated perhaps among popularizers of science. When simplicity, however, is regarded as an attribute of a system (rather than of a series), its resemblance to the simplicity of a work of art, which is not a simplicity of blank identity or sparsity of ideas, is more striking. If the suggestion made above that the simplicity of science is not so much a matter of counting postulates as an expression of what for lack of a better term we called "tact," the resemblance between great scientific theorizing and artistic creation is manifest.

(4) The metaphysical implications of the principle of sufficient reason are somewhat different if the approach is conventionalistic rather than otherwise. It strengthens the case against a metaphysics based merely on the positive results of science, because the principle of exclusion, which is the form in which the law of parsimony is used, is now regarded as itself a variable, dependent upon many factors besides the cosmological nature of the objects of science. And when the conventionalistic interpretation is substituted for the naturalistic and formalistic, it shows that the history of science can not be construed as a straight-line development in the direction of an absolute minimum of complexity.

It leaves entirely unanswered the question as to whether the metaphysical nature of the world is such as to make simple expla-

nations "true." It does not tell us whether simple ideas work because they are true or they are true because they work; it does not indicate whether the simple laws we have discovered have been disclosed merely because they are simple, or whether the entire natural organization is simple and homogeneous with that which we have brought to light. It introduces into science itself a variability and relativity (which has been suspected more and more in recent years), and this is a warning against absolutizing the results of science into a final metaphysic. In a word, it puts the problem of the ultimate nature of reality back where it belongs, in the hands of the metaphysician.

Human beings have seldom been at a loss to imagine sufficient reasons for things. The great trouble, if we may judge from the conflicts which occur in times of scientific crisis such as the sixteenth century and the end of the nineteenth, has been in getting rid of unneeded explanations. It is for this reason that, in critical turning points in the history of science, parsimony rather than adequacy has been the battle cry of the revolutionary forces. In the normal course of a science's growth, however, when to a stable axiomatic basis more and newer facts are assimilated, it is the requirement of adequacy which is dominant. And when a critical juncture comes, their applications modify and check each other so that nothing useful is lost in the process of simplification.

LEWIS WHITE BECK

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THREE VIEWS OF A CANDLE

HERACLITUS:

Observe this candle, for when you perceive
That, being yellow, it is red (although quite white and almost entirely blue),
You will know why we must always be strangers (or only friends in pattern);
Because—though we have more than once shut winter out with the same door, and bartered for the stars coming out as the day dwindled,
Even though our sandals are now gray with the same dust—
Perhaps tomorrow we shall have very little to say to each other.
Blow on the candle, and you will wonder at promise and prophecy.
You will know why, even as I roll the cool syrup of Truth on my tongue,

Lies fester in my mouth,
And why flame, even while sweeping the dark with wings,
Flaunts its skirts and soars . . . downward.

PYTHAGORAS:

I hope you will not mind, since I spent all one day (and burnt my finger) measuring this flame against infinity and counted nine fragments of the boundless, that I speak of this candle as nine.
At any rate, I contemplate candles every evening,
For, by watching flame whip darkness back into the shadows, it is immediately clear
That, like fire, my soul can delicately insinuate itself through my body (which in itself is only sense and substance—oh, the chill of the bone and the dark of the blood)
And purify: consume desire, diminish love and rage to ashes in the mouth,
Flare up into the caverns of my mind and cut the bond that tethers me here just beyond reach of the gods . . .
You see, I, also, know there is more to a candle than a light making a shadow;
Sometimes, even, when wind spins the fire thin and green,
I fancy I hear, like muted reeds,
Faint echoes of the music of the spheres.

EMPEDOCLES:

Look at the candle?
Why, I am that candle and more—
Never think that because smoke does not curl from my fingertips and there are no ashes under my lids,
That I am not compounded of fire—
(Even as air sings in me, too, and water that is not tears, and earth sifting through me in an uncharted pattern)—
Have you never been lost in a forest
And almost lost your identity?
Have you never hated the mesh which will not let you escape,
That muffles the river-voices in you,
And the breath that longs for the wind?
Would you not give your fire back to the sun,
And lose yourself in the cool earth, covering your mouth with leaves?

CAROL RENIUS

LOS ANGELES, CALIFORNIA

BOOK REVIEW

Romanticism and the Modern Ego. JACQUES BARZUN. Boston: Little, Brown and Company. 1943. ix + 359 pp. $2.75.

In these days of neo-classic programs and manifestoes Professor Barzun is well known as the doughty crusader ever ready to draw his sword in defense of the honor of the Romantic vision. In his Lowell Lectures he is still out for blood, the blood of the classicists and of the "modern ego." If the combat turn out not particularly gory, it is not for want of skillful thrusts. It is rather because, as Mr. Barzun disarmingly warns us, his opponents have been carefully stuffed with straw.

> Instead of trying to give a rapid but comprehensive notion of modernism, I want to deal only with what seems to me its intellectual vices, its irritating or perverse deficiencies. . . . The reasons for putting on blinders here are several. . . . I want to show by a reverse example the very method that many moderns use in judging romanticism—the suppression of essential facts and the exhibition of others in a vacuum that destroys their meaning. [P. 161.]

The ensuing *tu quoque*—which is turned against the "classicists" also—is stimulating and provocative enough; and Pound and T. S. Eliot, Lytton Strachey and the neo-medievalists, come off with their garments sadly awry. The thrusts are penetrating, as well as good fun. But Mr. Barzun is the first to insist that it is all hardly too serious, and certainly not the whole truth.

What is serious in the book is its sturdy defense of "romanticism," both the "historic" romanticism that flourished between 1770 and 1850, and the "intrinsic" romanticism that is an eternal human attitude. The volume is rather loosely organized, and the reason for the sequence of topics in the individual chapters is not always too apparent; a hundred of its three hundred odd pages are given over to "Notes and References," in which are embedded some of the author's sharpest barbs. But running through the lectures is the serious attempt at a fresh analysis of the traits and achievements of the romantic movement; and whether Mr. Barzun has here said the last word or no, he has certainly succeeded in rescuing those great figures and what they stood for from contamination by the muddy flood of vague and contradictory debased usages of the adjective "romantic," which he amusingly illustrates in one of his witty chapters.

Mr. Barzun wisely and soundly points out that "the one thing that unifies men in a given age is not their individual philosophies but the dominant problem that these philosophies are designed to solve." So instead of repeating once more the old mistake of try-

ing to find certain traits common to the great romanticists of the romantic period, he concentrates rather on the cultural *problem* of that post-revolutionary age. "This problem was to create a new world on the ruins of the old. The French Revolution and Napoleon had made a clean sweep. . . . Romanticism is first of all constructive and creative; it is what may be called a solving epoch, as against the *dis*solving eighteenth century." In this work of construction, men felt the need of fresh materials, and turned to experience to furnish them. Old forms had decayed and proved inadequate; new resources had to be found, and new forms built out of them. Thus in Mr. Barzun's analysis the romantic era displayed both a critical and a constructive moment: decadent "fixed systems" and absolutes were criticized by an appeal to living experience, and on the basis of that experience new syntheses were erected.

The romantic era was thus but one illustration of "intrinsic romanticism."

> We cannot dismiss the nineteenth century as the breaker of tradition when before it, in the ninth, twelfth, and fourteenth centuries, previous epochs of expansion and exploration followed upon attempts at fixed systems. . . . So far in the world's history, the periods of consolidation have been classicist; the periods of expansion and individual creation, romanticist. [Pp. 197, 208.]

Mr. Barzun sees an essentially "romantic" Renaissance, followed by a "consolidating" neo-classicism, the first synthesis of modern times. "It is precisely because modern classicism was an original creation, and not a copy, that it deserves to be called great. But it differed from romanticism in that it sought greatness through the adoption of common forms and attempted to make them exclusive." The second modern synthesis, romanticism, successfully escaped the "abstract and authoritative character" of the first.

Because it was a "cultural renovation," the romanticism of the romantic era displayed in life and in art the salient traits found in most of its great figures—a realism of fundamental intent in its art, a sense of the concrete and individual, an appeal to historical and personal experience of the world, a genuine concern for the sciences of the historical and the concrete, the biological, an immense productiveness. Mr. Barzun connects the romantic "cult of energy" with the needs of the period, and finds in this "sense of energy" the reason "why America is the land of romanticism par excellence and why her greatest philosopher, William James, asserted the doctrine in its fullness against all absolute, classical limits" (p. 187).

All this seems as sound as such historical generalizations can well be. But on one point Mr. Barzun's polemic zeal leads him

rather to blur his analysis. Romanticism conceived its creative mission in a certain way. "It conceived it in the light of a great contradiction concerning man. I mean the contrast between man's greatness and man's wretchedness; man's power and man's misery." And Mr. Barzun makes his "basic definition" of intrinsic romanticism run, "man's sense of his energy and of his weakness." In other words, Pascal is the great exemplar of romanticism.

Mr. Barzun is thinking of the "two souls in one breast" of *Faust*.

> In romanticism, the two souls can be variously interpreted. I have chosen as most basic man's double consciousness of power and weakness. Another way is to take it as the Christian conscience faced with good and evil. A third way is to view it as a conflict between man's sense of values and his knowledge that nature is indifferent. [P. 70.]

Pascal is chosen, of course, because Irving Babbitt chose him first: Mr. Barzun is nothing if not a skillful polemicist. But this concentration on the two souls as the distinctive *way* in which romanticists are critical and creative, enables him to prolong romanticism both into the Christian past and into the naturalistic present. On the one hand, "the romantic view of life is basically Christian" (p. 133). On the other, the last three of the "four phases" of romanticism become—realism, symbolism or impressionism, and naturalism, which are all romanticism "under different names," all efforts at specialization, selection, refinement, and intensification. Each is a reduction of romantic perceptions and methods to a single term, in the interest of the quicker or surer achievement of some of the romantic aims: nationality, intellectual unity, or the improvement of the human lot.

Mr. Barzun is thus enabled by his very broad conception of romanticism to trace certain continuities that are often overlooked. He does it brilliantly and suggestively; this is in fact the major contribution of his book to intellectual history, and it is an illuminating contribution. But he also has a device for gathering under the romantic banner most of the great figures of past and present. Obviously, it is equally easy to extend "classicism" also, as it appears in its major representatives, especially since, as the author points out, they themselves display most of the traits usually labelled "romantic." Of course he realizes this, and on his last page confesses, "I agree with those who say the quarrel of romanticism and classicism is one of words." His real "third choice" emerges at the very end as "the pragmatic pluralism of William James. . . . Pluralism means creating unities and uniformities only where and when useful, with no pretense that they

are eternal or divinely ordained." It was a great battle while it lasted, and Mr. Barzun remains in possession of the field. He can not, however, forbear a parting shot: "Romantic and classic are not the two poles of human thought, but the whole and the half."

Mr. Barzun is justifiably irritated by the facile assumption that Fascism and National Socialism are "expressions" of romanticism, and were "inspired" by the romantic thinkers of a century or more ago. He reminds us of some of the facts about Rousseau, Fichte, and Hegel that we are prone to forget—that Fichte, for instance, was an earnest internationalist, and that "for thirty years after his death, Hegel was considered throughout reactionary Europe as a dangerous liberal and a fomenter of revolution on behalf of the common man" (p. 45). His most effective way of disposing of such distortions of history is to ask the simple question, "whether the French Revolution and the Napoleonic regime were themselves expressions of romanticism"—a question that reminds us sharply of the enormous complexities of "the relations of politics to culture." It is not without malice that he turns the tables and casually refers to totalitarianism as "unitary classicism." In deference to our fighting comrades, the Russians are said to display "a Pickwickian classicism in combination with a factual romanticism."

Mr. Barzun's real weakness as an intellectual historian of modern times seems still to lie on the side of science. There is nothing in this book quite like the idiosyncrasies of his view of Darwin, elsewhere set forth, though he does say, misleadingly enough, that the "biological revolution" against the "absolute reign of physics and mathematics" brought a new perception, "vitalism"; and that biology implies that "life is an element and not merely a combination of dead parts. It implies, in short, organic structure and organic function" (pp. 76, 78). And he does agree with the romantic philosophers that "science can deal only with a restricted field of experience. Scientific method proceeds by exclusion and achieves wonderful results; art and philosophy are, on the contrary, inclusive disciplines" (p. 91). Such a view would have astonished his hero, James.

But it is not in such incidental vagueness and lack of precise analysis that Mr. Barzun's neglect of science is really serious. It becomes so rather in what it leads him to omit. He had a wonderful opportunity to illustrate the methods and achievements of the romantic temper, conceived as he has defined it, in the scientific enterprise itself. And I mean not merely such rather superficial observations as that with the romantic thinkers we too today have rejected "mechanistic materialism," and in De Vries and

Bateson, James and Freud, created a "scientific neo-romanticism" (p. 159). For surely science, like any art, can display equally the classic or the romantic temper. What is the enterprise of the scientific movement in the first part of the nineteenth century, with its eager gathering of experience and facts, with the formulating of its great generalizations, with its development of the experimental method and temper, but itself a cardinal illustration of the very "romantic spirit" he has been talking about? Instead of seizing this golden opportunity to drive his point home, however, Mr. Barzun treats science as something that merely "happened" to be going on while the great achievements of neo-classicists and romantics were being worked out. "The classical scheme of society," he remarks revealingly, "coincided with a great scientific epoch" (p. 57).

This neglect of the real importance of the tremendous intellectual fact of the scientific enterprise during the whole modern period is symptomatic also of a lack of concern with the complex dynamics of social and cultural change. "In the eighteenth century the stirrings of unchanneled emotion were the most tangible forces disrupting the old order" (p. 75). This is rather typical: cultural history appears as a debate between rival -isms and abstract "forces," with little attempt to get below the surface. But here, alas, Mr. Barzun the intellectual historian is only sharing the limitations of the modern ego.

With these reservations, and conceived in its own terms, Mr. Barzun's book is stimulating, provocative, and under its surface exuberance a sound analysis. He has proved his case for romanticism, though he has not tried to be fair to positions usually distinguished from it. His thesis granted, perhaps he can now go on to push his analysis further and deeper, and try to answer some of the more difficult questions involved in understanding the history he has so brilliantly set forth, which are here not raised.

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BOOK NOTES

The Moral Ideals of Our Civilization. RADOSLAV A. TSANOFF. New York: E. P. Dutton & Co., Inc. 1942. xix + 636 pp. $5.00.

It is Professor Tsanoff's purpose in this interesting and well-written book to give a general survey both of the development of ethical theories and of the "moral values and principles" of our culture, of our "spiritual heritage." This project is of more

than speculative interest to Professor Tsanoff, for he feels that a critical understanding of the roots of our culture may help to alleviate the "impoverishment of the inner man," the "stunted spirit," which so many critics have found to be characteristic of modern culture. The author, however much he may agree with the conclusion of these critics that the modern civilization "lacks wisdom," does not content himself with a nostalgic cry for a Periclean age or a medieval synthesis. Moral progress should go on from here; the face of modern man is too definitely turned to the future to permit any return. But a truly pious regard for the sources of our being will be of assistance in this development.

One of the remarkable features of this book is the breadth of its scope. Professor Tsanoff takes moral thought where he finds it, whether the particular moralists have traditionally had a place in standard histories of philosophy or not. With justice he notes that "moral reflection in our civilization has not been the monopoly of professional sages or professors of philosophy." He admits, of course, that he has had to be selective; it is perhaps unfortunate that he was not more explicit as to his principle of selection. Certainly it has not been anything so simple as the popular influence of the particular thinkers. Nor does the selection have any programmatic tendencies; it is too broad and tolerant for that.

The first fifth of the extensive work is devoted to a brief survey of ancient and medieval morality. In spite of its brevity the chapter entitled "The Moral Ideals of Classical Antiquity" is in many ways one of the best parts of the book. As he comes closer to the contemporary period, the author, with a sense of perspective, becomes more detailed in his treatment and less highly selective. The last half of the book treats of the nineteenth and twentieth centuries. If the author were particularly interested in development and continuity—and the whole manner of his treatment suggests that this is not his real interest—one might wonder that he could deal with this period as a unit. He has adhered, for the most part, to traditional divisions and distinctions: ancient, medieval, and modern, and within the modern period, pre-Kantian and post-Kantian. In this latter period, where traditionality of divisions deserts him, he turns to what is for the most part a static, rather than a dynamic presentation of moral ideas. In this, of course, he resembles most histories of philosophy, which tend simply to list post-Kantian or, at any rate, post-Hegelian philosophers. Certainly, with the lack of distance, it is difficult to do otherwise; perhaps it is impossible unless one simply fits them into a preconceived scheme. This last, at least, Professor Tsanoff is unwilling to do.

The net effect of this treatment, however, is to give credibility to the charge of the critics of modernity that modern moral thinking is indeed chaotic, devoid of leading ideas and ideals. The possibility must, however, be borne in mind that it may be the lack of distance which produces the appearance of chaos. The crowds at the wailing wall may be the result of the modern tendency toward introspection; it is at least not necessarily the case that this habit was forced upon us by moral disunity. Incidentally, this same habit of introspection, which the present reviewer feels to be an important factor in modern moral thought, and which makes the *Confessions* of Rousseau for him a sort of moral biography of modern man, is not emphasized to any extent by Professor Tsanoff.

When he comes to the consideration of the contemporary scene, the author is apparently either little interested or feels himself not qualified to provide more than a listing of moralists and a description of their approaches to moral problems. Perhaps he only feels that their contributions merit no more than such a brief mention. But the book ends on a note of optimism, a hope that the sectarianism which has always hindered moral speculation will fall before the objectivity of scientific treatment, yet with a warning that in this clarification there is a danger of confusing moral evaluation and scientific description.

Whatever defects or limitations are to be found in this book are ones which apply to all such extensive surveys. As a matter of fact such usual faults as misleading statements, resulting from attempts at brevity, are exceptionally few. While the present reviewer might disagree with some interpretations in detail, he is not conscious of having come across any which he would not regard at least as possible interpretations.

It is a book which would seem to be best suited for the interested layman. That judgment does not imply that Professor Tsanoff makes an attempt to be "popular" or to oversimplify. As a history of philosophy it has its obvious limitations. As a textbook in morals it would be no substitute for first-hand acquaintance with the source material; it might, however, find use as collateral reading in such a course, if its length were not felt to be a disadvantage. As an intellectual history it would have to include much more than the moral thought. But as what it proposes to be, a stimulus to thought about and understanding of the ideals which our culture has sought or followed, as an encouragement to read the works of the thinkers who are discussed, it is a valuable contribution.

G. C. S.

Twentieth Century Philosophy: Living Schools of Thought. Edited by DAGOBERT D. RUNES. New York: Philosophical Library. 1943. 571 pp. $5.00.

Sounding a mighty Aeolian blast on his editorial horn, Mr. Runes has summoned together the winds of doctrine. That some of them have been a little recalcitrant goes without saying. There are twenty-two essays in this volume, seven of which are selections from previous works by Russell, Maritain, Whitehead, Dewey, Santayana, Montague, and Roscoe Pound. Mr. Runes's editorial policy and purpose are ill-defined. From his brief preface I am not clear whether the presentation of the book to "the student" intends this term in the narrow or the broad sense, and obviously some of the contributors were not clear either. The essays directly contributed seem to fall into several categories: One is a survey of recent philosophizing in a given subject-matter (W. M. Urban on axiology); two are critical summaries of currently dominant concepts in a given subject-matter (D. H. Parker on the esthetics, V. F. Lenzen on philosophy of science); four are expositions of current movements (R. T. Flewelling on personalism, M. Farber on phenomenology, H. Feigl on logical empiricism, J. Somerville on dialectical materialism); two are analytical treatments of special problems (E. W. Hall on the status and definition of metaphysics, D. C. Macintosh on the relation between theology and metaphysics); two are accounts of historical influences (A. C. Ewing on Kantianism, R. Hoenigswald on Hegelianism); and one is a survey of philosophic traditions in a given culture (W. Chan on philosophies of China). The remaining three essays are most properly classified as miscellaneous expressions of personal opinion: J. H. Tufts writes on "Ethics," J. E. Boodin on "Philosophy of History," and R. B. Winn on "Philosophic Naturalism."

It can not be said that more than a minority of the writers achieve their purpose with reasonable success. The best of the contributions, it seems to me, is Feigl's. It is thorough, informative, and well organized, which is precisely what an article that purports to be representative should be. Parker's piece is suggestive but excessively short, and Farber seems to be determined that the reader should get no more than a scent of phenomenology. Lenzen, in a compendium that is good as far as it goes, does not sufficiently emphasize the existence of controversy on specific methodological issues, leaving to the uninitiated the possible impression that progress in the philosophy of science is analogous to that in the special sciences. Hall's essay, the longest in the volume, tries to arrive at a positive conclusion on a much-disputed

matter. Anti-metaphysical philosophies, he maintains, are surreptitiously metaphysical, and a clear conception of the purpose of metaphysics can vindicate its function without laboring under illusions. The review of contemporary value theory by Urban is a good illustration of a case history lapsing into a diagnosis. To the author's credit, however, is a perception of the difficulties involved in axiological concepts he finds congenial, like that of emotional intuition.

In the light of what are generally acknowledged to be twentieth-century emphases, some of the tendencies in this volume are surprising. Underlying Lenzen's discussion is a tacit conception of "science" as a collection of schemes or opinions. Disregard of its capacity as an attitude, direction, or method is responsible for his judgment, left unqualified, that it can not "pronounce" upon doctrines "such as those about the influence of supernatural agents on natural events." Or consider the view of Parker that "the best short definition of art remains therefore Shelley's 'the expression of the imagination.'" Not the definition itself so much as the persistent interpretation of "imagination" in exclusively esthetic terms is the antique that mars Parker's analysis. This is no merely terminological issue. An approach that differentiates so central a concept by "its intimate relation with passion; its kinship with dream; its contrast, nay opposition to reason" is at least indirectly responsible for much fruitless philosophic strife. A third and particularly unlovely persistency is Urban's uncritical yet conscious identification of value naturalism and value subjectivism. If he is writing for those familiar with axiological controversy, he ought to know better; and if he is writing for "students," he is guilty of corrupting the youth. If a fourth relic be desired, we need only turn to Hall's paper, where we find, in the face of endless protestation and reiteration by pragmatists to the contrary, that "pragmatism" is an "enthronement of action." To speak in this late day of pragmatism without specifying which pragmatism and whose version is sheer verbiage. Finally, it is difficult to understand why Feigl, in his employment of the terms "experience," "experiential reference," and "empirically confirmable," is not aware of, or does not choose to consider, their equivocal and question-begging character. Easier to understand but no less interesting to observe is the fact that certain positions of logical empiricism, like that relating to the analysis of ethics, originally born in a spirit of critical reconstruction, have blossomed into rigid dogmas. It has not been established by logical empiricists that a greater degree of humility would be incompatible with clarity.

A good deal of useful information, factual and otherwise, resides in this collection. On the whole, it is an effective exhibition of philosophic diversity. It is much less illuminating as a conspectus of basic directions in this century, or as an explanation of influences and cross-currents.

J. B.

NOTES AND NEWS

We have received the following news from the University of Chicago:

Professor William O'Meara is visiting Assistant Professor of Humanities, in the College, for the year, on leave from Fordham University. Professor Fritz Marti is visiting Lecturer in Humanities, in the College, for the year, on leave from the University of Maryland. Professor T. V. Smith is on leave of absence for military service, as lieutenant colonel in the army, and is now somewhere in North Africa. Professor Rudolf Carnap is assigned to research for the year on a Rockefeller Foundation grant. Frank H. Knight, Professor of Social Science in the University of Chicago, is now a member of the department of philosophy. Associate Professor Charles W. Morris is assigned to research for the year on a Rockefeller Foundation grant.

Dr. Robert F. Creegan, formerly Professor of Philosophy and Psychology at Cumberland University, Tennessee, has been appointed Acting Assistant Professor of Philosophy and Psychology at the University of Mississippi, substituting for Professor Harold Hantz, who has entered military service.

"Value in a World of Fact" is the subject of a symposium to be held by the Conference on Methods in Philosophy and the Sciences at the New School for Social Research, New York, on November 28, 1943. At the morning session, devoted to "Value and Fact in Philosophy," the speakers will be Professor Wolfgang Köhler of Swarthmore College, and Dr. Richard M. Brickner of the Neurological Institute, New York. "The Place of Value in the Social Scene" will be the topic of the afternoon session, at which the speakers will be Professor Wesley C. Mitchell, of Columbia University, and Mr. Justice Thurman Arnold, of the United States Court of Appeals, District of Columbia.

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